pared with the analytical methods and more flexible notation adopted by the followers of Leibnitz.

The Leibnitzian notation, although originally connected with the doctrine of infinitesimals, has now been universally accepted; so that we must inevitably denote an absolute velocity by $\frac{dx}{dt}$, and a relative velocity by $\frac{dy}{dx}$. The question which is still, as it seems to us, debatable, is whether these symbols shall be defined (1°) by the conception of a velocity, (2°) as limits of finite differences, or (3°) as the ratios of infinitesimal differences. The second course arose as a protest against the logical difficulties involved in the conception of infinitesimals: it labors under the disadvantage of attaching no separate meanings to the symbols dx, dy, and dt, and thereby loses much of the advantage of the Leibnitzian notation. This method is best exemplified in the excellent treatise of the late Dr. Todhunter. On the other hand, the employment of the notion of rates in the fundamental definitions enables us to give to the detached symbols dx, dy, and dt, definite meanings which are not of necessity infinitesimal.

It appears to us that this method of presenting the subject is better adapted than that of limits to the purposes of elementary instruction. We do not attempt or desire to dispense with the use of limits, as the following quotation from our preface will show:—

"The distinction between the view of the differential calculus here presented, and that found in most of the standard works on the subject hitherto published, may be stated thus: the derivative $\frac{dy}{dx}$ is usually defined as the limit which the ratio of the finite quantities Δy and Δx approaches when these quantities are indefinitely diminished. When this definition is employed, it is necessary, before proceeding to kinematical applications, to prove that this limit is the measure of the relative rates of x and y. In this work the order is reversed; that is, dx and dy are so defined that their ratio is equal to the ratio of the relative rates of x and y: and in chapter xi., by applying the usual method of evaluating indeterminate forms, it is shown that the limit of $\frac{\Delta y}{\Delta x}$, when Δx is diminished indefinitely, is equal to the ratio $\frac{dy}{dx}$. Thus the employment of limits is put off until we are prepared to show that the limit has a definite value, capable of expression in a language already familiar to the student."

Our experience has been, that the student trained by this method finds no difficulty in passing to the employment of infinitesimals, in obtaining the differentials which are required in the mechanical applications of the integral calculus; for example, those required in the determination of moments of inertia, resultant attractions, etc.

J. M. RICE.
W. W. JOHNSON.

U. S. naval academy.

Silk-culture in the colonies.

In your review of my census report on silk-manufacture in the United States, your critic takes issue with me as to the amount of silk raised in the colonies. He declares that there is a tendency on my part "to depreciate the quantity and quality of silk produced,—a tendency which is natural, and doubtless unconscious in an agent of manufacturers." In support of this grave imputation, your critic adduces two points on which he disputes my proof that certain estimates, hitherto accepted as relating to raw silk, really refer to cocoons, and probably to fresh cocoons. He says, first, that I by no means make it clear that the term 'raw silk balls' really meant cocoons, "as it might apply to the twisted hanks of reeled silk, and the term 'cocoons' was in use at that time." To this it need only be said, that, in the literature of the colonial period, cocoons are frequently designated by the term 'balls,' or 'silk balls.' For instance:—

"Removing your branches from the tables, and your silke-balls or bottomes from the branches 5 dayes after the worke is perfected, the balls are then to be made election of for such seed as you will preserve for the year following. Bonoeill and De Serres do both agree that there should be proportioned 200 balls for one ounce of seed, the balls male and female."

On the other hand, in a widely extended reading on the subject, I have never met with the term 'balls' as signifying reeled silk in any form; and I have no reason to believe that reeled silk was made into balls.

Your critic remarks, secondly, "It is certainly not justifiable to assume that the cocoons were necessarily fresh, as they are not thus handled and marketed." They are so handled and marketed at the present day. Statistics of production in European countries and districts are compiled, based on the weight of fresh cocoons. The commerce in them is very large. Quotations of their market-prices appear, during the season, in trade reports and journals. For instance: in the *Moniteur des soies* of June 30, 1883, under the headings 'Prix des cocons Français' and 'Marchés des cocons Italiens,' there are pages of this sort of information; and it is so well understood as referring to fresh cocoons, that no special designation is used for them: they are simply 'cocons.' Indeed, I am assured, on good authority, that it is only fresh cocoons that go from the producers to the filatures: even if 'choked,' they are accounted fresh.

Is it not justifiable to believe that estimates of the weight of cocoons produced in Georgia, and of what was sent to the filature there, were similarly made; that is, that they referred to fresh cocoons? This view of the case came to me only after months of research and final good fortune in tracing the origin of an historical error. Until then, I had accepted without question the current histories in their accounts of silk production in the colonies. My explanation reconciles their strange discrepancies: before refusing it, should not some other solution be offered?

While differing wholly from the conclusions of your article as to the causes of failure and cessation of silk-culture in this country, I should not have troubled you with a reply to criticisms on my work, had they not contained the imputation to which, with great regret, I have deemed it necessary to refer.

WM. C. WYCKOFF.

Rainfall at Amherst, Mass.

The month of February, 1884, stands alone upon the meteorological record of Amherst college in showing an average cloudiness of seventy-seven per cent of the sky. During the forty-two years which this record covers, in no previous case has the cloudiness of a month averaged more than seventy-four per cent; in only five cases has it reached seventy; the range generally being between forty and sixty, and the mean almost exactly fifty.

The fact that clouds and fog gather only in air containing particles of dust, which has been scientifically demonstrated, suggests the question, whether the volcanic dust from Krakatoa, which in higher air gave to us the brilliant evening skies of December last, may not, in its gradual descent toward the earth, have reached in February the lower level, in which our clouds are formed, and have been the cause of this unprecedented accumulation of them.

S. C. SNELL.

Amherst, Mass.